The Alongshore Tilt of Mean Dynamic Topography and its Implications for Model Validation and Ocean Monitoring

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Response to Comments by Reviewers

We thank the reviewer again for their insightful comments – we have responded to all of them as detailed below. The reviewers' comments are repeated in bold font, and our responses are given in regular font with modified parts of the text, including the respective line numbers, by italicized font. We believe the changes have resulted in a significant improvement to the manuscript and hope the reviewer and editor will find the revised version suitable for publication.

Reviewer 1

The authors have made substantial changes to the manuscript. The mathematical derivations are now more succinct, and the rearrangement makes the logic of the arguments much clearer. I would like to thank them for these clarifications and responses to my (and the other reviewer's) earlier comments. The paper is greatly improved.

Thank you again for your very insightful comments and constructive feedback on our manuscript! Following your suggestions, we have changed the wording around the interpretations and addressed your comments as detailed below.

However, I do still have some significant problems with the interpretation of the "regional view" analysis, which I feel really does still need some further changes before it can be considered ready for publication. There is also one issue with the coastal interpretation, and some other minor issues which I list below.

The maths of the regional view is correct, and the analysis is interesting. In particular, it is intriguing that the JEBAR term appears so dominant in this analysis, in both mean state and time dependence, when the coastal balance is so dominated by the winds. I am persuaded that the presentation of the Csanady model is worthwhile for the insight it gives into both wind forced and "downstream" responses for the barotropic case, as a contrast to what is actually seen in the model.

The problem is with the interpretation as "upwelling", and the descriptions in various places referring to vortex stretching. I have tried for quite some time to find a good physical interpretation of the balance that has been presented, and have not come up with anything clearer, but what is clear is that w* is not related to upwelling in any meaningful sense.

To make this clear, consider a few examples.

1) Consider a wind stress which does not intersect the coast, and has no curl. This will drive a recirculating Ekman flux with no divergence, and so the only flow will be this Ekman flux. From (19), u* is then the Ekman flux divided by H, and w* from (20) can be positive or negative, or zero, depending on how the topography is orientated relative to the Ekman flux. In this scenario there is no upwelling or downwelling. There will also be no coastal sea level signal. So w* can be nonzero when w is zero.

2) Close to the coast in the diagnostics presented here, the primary balance is between wind stress (an Ekman flux away from the coast) and pressure gradient (a geostrophic flow towards the coast), with bottom stress a minor contributor in most places, and no depth-average flow toward or away from the coast. It is also close to barotropic, so geostrophic u and bottom geostrophic u are equal in (19) making u* a flow along the coast (along isobaths), and therefore w* is zero from (20). However, this represents an upwelling flow as the geostrophic flow toward the coast is at greater depth than the Ekman flux away. So w can be zero in an upwelling flow.

3) In a region where the flow does not reach the seafloor (so the geostrophic component can be computed by thermal wind relative to the bottom), and a wind-driven Ekman flow away from the coast is balanced by a geostrophic flow towards the coast, the depth-averaged u and bottom geostrophic u in (19) would both be zero, so u* would be minus the thermal wind flow, making w* -ve from (20). But in this region there is no vertical velocity (no convergence of the Ekman flux), and the oceanward surface Ekman flux balanced by a coastward geostrophic flow at greater average depth actually implies an upwelling somewhere coastward of this region. So w* can be nonzero when w is zero, and the flow is not confined to the surface Ekman layer.

4) The diagnostics shown in Fig. 8 illustrate a case in which the JEBAR term (coming from geostrophic u minus bottom geostrophic u in (19), or thermal wind flow relative to the bottom) dominates, and is larger than the depth-averaged flow effect. In that case, from (19) and (20), w* is negative as observed if the thermal wind flow is towards the coast. With the wind-driven Ekman flow being away from the coast, this is clearly an upwelling scenario similar to 3) above.

5) In the Csanady model the JEBAR term is absent. In this case, it is only the depth-averaged flow that matters in (19), and w* can be diagnosed from the streamlines in Fig. 5, being positive in the upper part of the figure where the depth-integrated flow is toward the coast and negative in the lower part of the picture. But there is no wind stress curl in this configuration, so w is zero near the surface, and a downslope geostrophic flow requires vortex stretching, and hence w negative through the interior of the flow where the geostrophic flow is downslope, and vice versa. The right hand panel shows that this geostrophic flow is (in the sense of towards or away from the coast) in the opposite direction to the depth-integrated flow in the northern region and in much of the southern region, so the actual w in a large part of the flow has the opposite sign to w*. On the other hand, with southward geostrophic currents everywhere, the bottom Ekman flux will be away from the coast everywhere and thus w will be negative in the bottom Ekman layer. If this layer has sufficient weight in the chosen form of vertical average, it will make w* have the same sign as vertically averaged w in the southern region, but will exacerbate the mismatch in the north. So a full solution can have different relationships between w* and w in different regions.

From the above examples it should be clear that there is no meaningful sense in which positive w* can be held to represent upwelling. It seems more likely to be a downwelling, but is not in general related to the vertical velocities in any consistent way. In the same way, w* has no consistent relationship to Ekman pumping or vortex stretching (see example 1 above), and interpreting the curl(tau/H) term as Ekman pumping is incorrect, since it includes both wind stress curl and a term tau x grad(H) which represents the Ekman flux across depth contours, which need not be associated with any vertical velocity.

It is unfortunate that there seems to be no simple interpretation of u*.grad(H), but the results associated with it do seem very interesting. In particular, Fig. 8 shows that the term which is absent in the Csanady model is actually dominant in this simulation, which is interesting for such a shallow region (perhaps a measure of the strong freshwater influence from river input?). It is a particularly striking contrast to Lentz (2024). I suggest removing any mention of upwelling, and removing the w* notation which misleads through its implied relationship to w. Other, similar phrases such as "area integrated vortex tube stretching" (line 489) and "associated net Ekman pumping velocity" (line 333, see also 397) should also be removed as they are inaccurate. Similarly, the introduction of xi (line 495) is unhelpful, and the diagnostics in Fig. 8 should be presented in terms of sea level change, to remain consistent with other diagnostics, and to ensure that the scale is meaningful.

I would like to see these results kept in the paper because they are very interesting, even if a clear interpretation cannot be reached. But it is important that the misleading interpretations be removed.

Thank you for your detailed examples – we recognize that, unfortunately, there is no simple interpretation of $u^* \cdot \nabla H$, in particular with regards to upwelling. Your efforts to find a good physical interpretation are much appreciated! We have addressed your concerns by making the following changes in how the results are framed in the manuscript:

Based on your suggestions, we have removed the definition of w* (previous Eq. (20)). We furthermore removed any links between the regional view and upwelling in the interpretation of the GoMSS output and also removed any inaccurate phrases mentioned in your comments. Instead, we now use the phrase “the tilt provides a measure of area-integrated nearshore circulation” and variations thereof.

Additionally, we removed the definitions of ξ (previous Eqs. (33) and (34)) and express the diagnostics shown in Figure 8b in terms of $\Delta\eta_c$.

Another small but important issue is in the interpretation of the coastal balance, starting around line 425. Here, and elsewhere, it is stated that bottom friction partly balances wind stress. In fact, Fig. 7 shows that it acts in concert with wind stress in most regions. This is consistent with the fact that the Nova Scotia Current flows in the opposite direction to the wind stress.

As suggested, we modified the text to reflect the fact that the alongshore tilts due to wind stress and bottom friction have the same sign along most of the coast line of Nova Scotia. The new text reads:

"Along most of the coastline, this wind-driven tilt acts in concert with bottom friction ($\Delta\eta_{BF}$) acting on the Nova Scotia Current flowing in the opposite direction of the wind stress at the coast. An exception to this is the region around Sydney where bottom drag is strongest and associated with the Nova Scotia Current flowing close to the coast as it exits the Gulf of Saint Lawrence." (l. 427)

Minor points

Line 35: In fact Huang only says the accuracy can be at the centimetre level for low-lying coastal regions, and errors can be decimetric on mountainous coasts. It would be worth qualifying this statement.

We added a qualifying statement and the updated text reads:

"In coastal regions with flat topography, these independent measurements are accurate on the centimeter level (Huang 2017) and thus provide potentially valuable information for the validation of ocean and shelf circulation models." (l. 35)

Lines 265 and 353: As above, the curl(tau/H) term is a combination of a "torque" (in the same sense as used in the term bottom pressure torque) and an Ekman flux across depth contours.

Changed to:

"The remaining terms in (6) describe the curl of the difference between wind stress and bottom friction and their associated Ekman transports across depth contours, as well as vorticity dissipation due to lateral mixing." (l. 267)

"The combination of the net torque exerted by the wind stress and bottom drag (right-hand side) and their associated Ekman transports across depth contours, is balanced by vortex stretching/squashing through movement into deeper or shallower water, respectively." (l. 354)

Line 379: "Convergence of this onshore flow implies downwelling that is balanced by return flow in the frictional bottom boundary layer" is confusing. The wind-driven Ekman flux at the coast implies downwelling but this is only partially balanced by a frictional return flow. Some of the return flow is geostrophic, and some does not return but flows along the coast. The total onshore flow (which is what matters for u* in this model) is not balanced by any offshore flow by definition, but by a divergence of the alongshore flow. Please clarify.

We clarified this statement and the new text reads:

"The onshore wind-driven Ekman transport implies downwelling at the coast that is partially balanced by a return flow in the frictional bottom boundary layer and partially by an offshore geostrophic flow in the interior. The total onshore flow is balanced by a divergence of the

alongshore flow. The rate of water exchange between the surface Ekman layer and the interior of the water column in that area can be monitored by observing the sea level at the coast." (l. 381)

Line 437-8: It is only the boundary condition of the Csanady model that is justified by these diagnostics, not its application in the interior.

We added the qualifying statement *"as coastal boundary" (l. 441).*

Line 502: typo "with and"

This sentence has been removed.

Line 518-9: I don't follow this statement. Is it in relation to the curl(tau/H) term again? If so, it is a misinterpretation as the vorticity input is only due to curl(tau).

We clarified this statement. The new text reads:

"Although the wind stress can be uniform over a large area, the associated Ekman transport toward the deeper region offshore leads to a change in relative vorticity." (l. 512)

Line 554: please change "wind setup" to "coastal sea level slope due to alongshore wind stress"

We changed "wind setup" to *"tilt due to alongshore wind stress" (l. 547)*